

## ***Interactive comment on* “Estimating a relationship between aerosol optical thickness and surface wind speed over the ocean” by P. Glantz et al.**

### **Anonymous Referee #1**

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#### General:

The work presented analyses a 1 month SEAWIFS scene in the Central East Pacific to relate retrieved aerosol optical depth (AOD) to wind and humidity fields derived from the ECMWF. The article suggests that a simple regression can be established between sea salt AOD and surface wind speed. Applicable eventually everywhere and allowing to "go directly from wind speed to the aerosol-radiation impact on climate".

Even though the authors put some word of caution on this suggestion, the discussion is largely hiding that this simple relationship between wind and AOD might not exist. Without a much more solid discussion, this paper should not be published in ACP. It serves certainly as a discussion paper and is interesting in parts, but requires a major

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revision.

Major concerns:

Sea salt aerosol once produced is transported, depending on size. Such transported sea salt will provide a varying background to any local directly source related sea salt. An understanding of a background contribution from transported sea salt needs to be included in any relation between local wind speed and AOD. Work from e.g. Gong et al. 97 have shown problems in relating surface concentrations to wind speed. Transport models have thus abandoned the idea of using source functions which use wind speed derived concentrations in the lowest layer as boundary condition. Almost all global transport models of today resort to flux calculation of sea salt mass and number. AOD is even more dependent on transported sea salt than surface sea salt concentrations. AOD is related to humidity and to the fine particle spectrum with its inherent potential to be transported away from the source. The transported component contribution to the wind-AOD relationship is not really discussed in this article. How large is it? How variable is it?

The computation of sea salt AOT "according to environmental quantities" is used as "validation" of the relationship between wind and AOD. I don't see why the authors avoid saying that they estimate sea salt AOD with a box model, assuming steady state of fluxes. The inherent problems with using such box models does not allow to "validate". Again a transport model result would be much more powerful to investigate the usefulness of the simple wind-AOD relationship. In any case, validation as word should be avoided, comparison would be a much more adequate terminology.

Other background aerosols may have provided a background contribution to AOD. The Aeronet measurements from Mauna Loa indicates an average background AOD of roughly 0.01. At times this is almost 20% of AOD at the sea level Aeronet sites shown in the article. It would be interesting to see how much sulphate AOD is predicted by state of the art global transport models in the region. Any relationship pretending to

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link sea salt aerosol to AOD should be corrected for other background aerosols. The role of ammonium sulphate is not clearly discussed in the article. The offset in the wind-AOD relationship of 0.06 needs to be explained.

The influence of the upper threshold of AOD of 0.2 on the wind-AOD relationship should be quantified.

The paper in general is not well written in quite some parts. In the present state, the quality of the text alone does not permit a transfer to ACP. It should be carefully rewritten and shortened.

Detailed remarks:

abstract: "the validation ... is based on previous parameterization" the wording "validation" should be removed from all text.

" a factor of 2 higher mean AOT ": higher than what?

"is more or less supported by the validation" ???

"approximately 50% of the enhancement seems to be due to hygroscopic growth" Should be revisited after proper discussion of the ammonium sulphate contributions. The enhancement is also depending a lot on the actual humidity in the study area.

section 4.3 "Estimating AOT according to environmental quantities" 1)The terminology needs to be changed. It is confusing. "Estimated" AOT can also refer to the SEA-WIFS retrieved AOT. The Aeronet AOTs are also results of an inversion computation. "Computed" and "estimated" applies to all sorts of AOT used in this article. I propose to make more clear which AOT is discussed by referring either to "SeaWIFS retrieved AOT" - "Box model derived AOT" or "Aeronet retrieved AOT" in text and figure captions 2) What is really done here is proposing a box model calculation of boundary layer concentrations. It should be named and discussed as box model assumptions. "According to environmental quantities" is misleading.

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"to estimate the absolute changes in AOT .. without hygroscopic growth the expressions 1+2 are used..." The description is not clear and confusing: "Without hygroscopic growth"? But the GF for the lowest wind speed class is applied!?

"RH1 and BLMH 1 are the mean relative humidity and boundary layer height, respectively," Mean RH for the whole area, the whole time period? For any individual data point? How different is RH1 from 80%?

"for the ammonium sulphate particles it is only a relative change in AOT that is estimated and the first term is based on a work by Charlson." Don't understand.

chapter 5.1. "Furthermore,furthermore..." ??

Figures 5 and 6:

I think the Y coordinate should start at 0. ! Both figures should have the same y-scale!

Figure 6: I do not understand why the star and square for the wind class 0-1m/s are different. In chapter 4.3 it is written, that "this study assumes that the MBL over the North Pacific contained mainly ammonium sulphate particles during the low wind speed conditions..": How much sulphate is assumed? Note, there is no "AOT (Ui) for sulphate" computation described, equivalent to AOT-SS derived in equation 1 and 2. So does AOT-SO4 vary at every point? Wouldn't it be good to show in the figure 6 all AOT contributions from SS-AOT-dry, SS-AOT-RH growth, SO4-AOT-dry, SO4-AOT-RH growth It seems to me that all data are available.

section 5.2 "A correlation coefficient  $R^2=0.59$  is obtained according to the values denoted by the stars and the power fit shown in Fig. 6 obtained where the latter curve is the same as the one shown in Fig. 5." Not clear. What is correlated with what? The straight curve is not explained in the figure caption. Stars are used both in figure 5 and 6, which is confusing. But for different data.

"Thus, the difference in retrieved AOT with a factor of 2 increase obtained between the lowest and highest wind speed values shown in Fig. 5 is more or less supported by the

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results shown in Fig. 6. " More or less supported is not "validated" as mentioned in the abstract.

"Furthermore, the results suggest that approximately up to 50% of the enhanced AOT seems to be caused by the hygroscopic growth of the marine aerosols": How large is the sulphate growth contribution?

section 5.3. It is not really clear why this section is introduced. I can imagine reasons, but they are not really explained.

section 5.4.

This section requires a major tidying up of the English! It seems to me also long. Although it misses a quantitative discussion of the possible errors in the relationship  $AOT=f(u)$ . How will a relationship look like in the real world? In which direction will it change?

"In any case, the assumption that the marine aerosols grow to larger particle sizes due to water uptake and consequently influence the direct radiation back to space significantly is supported by the validation of the present result." ??

"Even so, the supermicron particles as well as the hygroscopic growth and boundary layer height are all expected to be highly sensitive to the local surface wind speed." ??

"For the third the present approach to estimate AOT probably also induces uncertainties that could be significant." ??

6. conclusions:

"Nearly a factor of 2 higher AOT is obtained for a wind speed up to about 13ms- over remote ocean areas." higher than what?

"A factor of 2 higher AOT is more or less supported by the validation of the results." As mentioned above, validation is not the right wording.

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"The results presented in this study suggest a significant direct radiative effect over the North Pacific." No radiative effect is discussed nor computed in the paper before this point.

"However, the radiative effect could be lower over oceans where organic species, with lower hygroscopicity, are internal mixed with sea salt." Pure speculation at this point. I don't think this is a useful discussion.

"For this purpose the fit in Fig. 5,  $AOT = 0.06 + 0.00025 \cdot U^{2.1}$  (4) may serve as a first one-line parameterization of the whole complex chain of steps from breaking waves to aerosol backscatter." This is an unjustified suggestion. First - the contribution from ammonium sulphate is hidden in this formula. Second - it is not tested in other regions of the world. Third - it underestimates probably the wind speed dependence (AOT retrieval cut off at 0.2; transported sea salt tends to dampen the relationship to an unknown degree). Fourth - it neglects the possible impact of different humidity regimes, vertical mixing regimes, the impact of wet removal processes. Last - it should be compared with results from transport models not just a box model.

"...but in view of the world-wide well behaved number or mass behaviour, we should perhaps dare to be optimistic also on this point." The world wide behaviour is not shown!

"it may be possible to skip many complex steps when evaluating the climate effects of the primary marine sea salt aerosol and go directly from the local wind speed to the aerosol-radiation impact, even though it is to early to say if the Eq. (4) must somehow be modified to apply to other oceans, seasons or similar." Contradictory. And, why should we at this point go back to this type of simplified parameterizations used in terms of concentrations in climate models 10 years ago?

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11621, 2006.

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